Vol. 25, No. 10 PSYCHOLOGICAL REVIEW PUBLICATIONS October, 1928

Psychological Bulletin

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PUBLISHED MONTHLY
For The American Psychological Association
BY THE PSYCHOLOGICAL REVIEW COMPANY
372-374 BROADWAY, ALBANY, N. Y.
AND PRINCETON, N. J.

Entered as second-class matter at the post-office at Albany, N. Y., September 25, 1922

Psychological Review Publications
of the
American Psychological Association

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PSYCHOLOGICAL REVIEW COMPANY
PRINCETON, N. J

Vol. 25, No. 10 October, 1928

THE
PSYCHOLOGICAL BULLETIN

CUTANEOUS AND KINAESTHETIC SENSES

BY JOHN T. METCALF
University of Vermont

The recent literature in the field covered by this review is mainly experimental in character, though some articles of a nonexperimental nature have appeared. Pauli's contribution (**44**) is a critical appreciation of Katz's *"Aufbau der Tastwelt."* The results of the latter's experimental work are discursively presented and carefully appraised. The discussion is divided into four parts as follows: (1) The observation and description of touch experience, (2) An analysis of the condition giving rise to touch experience, (3) Sensations of vibration, (4) Applications. Nogué (**41**) is concerned with the tendency we all have to symbolize the differences in quality of sensasions in terms of space. He explains this tendency on the ground that there is an order in both sensational quality and space, constituting a common element between them.

Several articles are devoted to the description of new apparatus and methods. Baron and Meifried-Devals (**2**) describe a new form of aesthesiometer for use in the investigation of the relationship between the duration and the intensity of tactile sensation. The instrument is electrically actuated, and may be controlled by a chronoscope. It is admirably planned to give a thorough control over the stimulus, and its inventors claim that it has given complete satisfaction in use. François (**16**) describes a method of applying thermal stimuli to the skin without stimulating the skin mechanically at the same time. The method involves the use of a radiant heat directed by a parabolic mirror, the intensity of the rays being controlled by an episcotister. Pendleton (**45**) describes a new method of marking temperature spots on the skin. The device consists essentially of a needle with its point dipped in printer's ink. When applied to the

skin it leaves a tiny point of ink which does not run or smear. The needle is attached to the metal thermal stimulator in such a way that by pressing a rubber bulb the experimenter caused it to drop so that its point touches the skin ½ mm. to one side of the point of the thermal stimulus. After the temperature points of an area have been located and marked in this way, a map of the area may be taken off on a piece of paper, on which, of course, the points appear in reverse order. Twitmyer and Fernberger (60) describe an improved form of heat grill made of copper tubes. A similar grill is used by Burnett and Dallenbach (9) in an experiment on heat to be considered below. A new type of apparatus for the production of sensations of vibration is described by Noldt (42). A wooden block is vibrated by means of an electromagnet which is separated from it by a thick layer of rubber. The chief advantages of this arrangement are that the apparatus is noiseless and the vibration frequencies are readily controllable. Bernstein (7) describes a new piece of apparatus for the study of movement, the "kymocyclograph." It is an improvement on the method of taking a series of photographs of an intermittent light attached to the moving object or member. The author says that the apparatus was designed for the study of the most varied forms of normal and pathological movement. Renshaw and Weiss (50) describe an apparatus for recording changes in the center of mass of the body under various stimulating conditions. The authors themselves have arranged a pursuit task as a stimulus. The recording device described seems to be a marked improvement over other methods. The recording is done by "Inkograph" pens on moving paper tape of the kind used in cash registers.

Interest in Head's theory continues, and several writers discuss it or deal with the phenomena to which it relates. Stopford (56) agrees with most of Head's conclusions. He would, however, distinguish two systems of deep sensibility as well as of cutaneous sensibility. Following Head's lead, he would relate one system of cutaneous sensibility to the cortex and the other to the thalamus, and he would make the same distinction between his own two systems of deep sensibility. He offers a new explanation of the two-stage recovery of sensibility. This explanation is based on the assumption that a more complex readjustment is required for cortical fibers than for thalamic ones. If a fiber of one kind joins a fiber of another kind in the process of regeneration, reëducation is necessary. This process of reëducation will take a much longer time than that required for the reappearance of some form of crude sensibility

depending on the connection of the sense organ with the thalamus. Piéron (**46**) believes that the researches carried out since Head's work was done have shown no evidence for distinct protopathic sensibility except the hyperalgesia which appears in areas deprived of their normal innervation. He thinks that the observations which led Head to assume the existence of protopathic sensibility are forms of pain, or better of algesic reactivity. This quality is affective, and very intense, of the kind which is revealed by "causalgia" in peripheral nerve lesions. Piéron criticizes Head for holding that localization and two-point discrimination are functions of an epicritic sensory system. This is to confuse sensory processes with higher ones, treating processes of intellectual identification and associative recognition as if they were sensory processes. Parsons (**43**), on the other hand, regards Head's distinction of epicritic and protopathic sensibility as fundamentally correct, despite difficulties in details. He prefers, however, to substitute the term "dyscritic" for protopathic. He considers the comparative anatomy of vertebrates, and finds that the primitive apparatus of the tactile sense can be recognized from amphioxus to man. He thinks it probable that epicritic sensibility proper does not appear below the phylogenetic level of the amphibia. Duthie (**15**) reports observations on deep sensibility in cases of nerve injury. Head held that deep sensibility was mediated by nerve fibers which ran with motor nerves and extended to their destination along tendons. The cases studied by Duthie were twenty cases of lesions involving cut tendons. The chief change resulting is a lowered sensitivity to pressure. Except for this, very little change in sensibility is found, and the author concludes that the nerves in question are less concerned in deep sensibility than Head thought.

Kiesow (**32**) returns again to the question of the validity of Weber's Law in the field of touch when the stimulus is confined to a single touch-spot. This study, like the former ones on the same subject, was carried out by Gatti under Kiesow's direction. A new type of aesthesiometer was constructed especially for this investigation. A hair point is mounted in the end of a small metal bar. This in turn slides vertically in a holder, and has at the upper end a little pan for weights. The pressure which the hair point exerts upon the skin is determined by the amount of weight with which it is loaded. Two of these instruments were used, one as the standard, the other as the variable stimulus. The weights were placed in the instruments by the experimenter, who then handed them one after the other to the observer who applied them to a pressure-point marked on his

own skin. Determinations were made for 3, 5, and 7 g/mm. The relative threshold is found to be practically constant, its value being about 1/7. This agrees perfectly with Gatti's earlier results and confirms Kiesow's previous conclusion that Weber's law for touch must depend upon intensive rather than extensive changes of the stimulus.

A thorough study of the qualities of tickle and itch has been made by Nafe (40). The stimuli were shreds of paper, bits of feathers and metal, a piece of coarse string with frayed end, small brushes, and the tips of the fingers. Many different parts of the body were worked on with the skin dry, wet, or oiled. The author concludes that the range of experiences from weak tickle to severe itch is unbroken. Tickle is different from itch chiefly in being less intensive. The author finds evidence that tickle is produced only where there is double stimulation, and concludes that there are no tickle spots and there is no special nerve supply for such experiences. Sullivan (58) studies the cutaneous perceptions of softness and hardness. The most important condition of softness is found to be warmth, the most important condition of hardness, cold. In addition, hardness is found to involve an even pressure and a well-defined boundary line, while softness involves an uneven pressure and an indefinite boundary line.

Ahringsmann and Buch (1) find that the pressure necessary for the cutaneous perception of a moving stimulus is only half that required in the case of a stationary stimulus. Moved stimuli, above the movement-pressure threshold but below the pressure threshold, are perceived on the pressure points as brief disturbances without extension. Moved stimuli above the pressure threshold give the impression of movement with the correct perception of direction. Hulin (30) experiments on the illusion of apparent movement in the tactual field. He uses several temporal intervals and several spatial separations of the stimuli. The seven subjects made a total of 13,500 judgments, and of these 29.7 per cent represented experiences of some sort of apparent movement. The optimal time relationship between the two stimuli for the production of apparent movement was found to be one in which the second stimulus was applied 75σ before the first one was removed. The several spatial separations were found to give the appearance of movement with uniform frequency. Qualitatively, the introspections show that apparent tactual movement consists of a perceptual complex involving pressure irradiation, perseveration, visual imagery, and kinaesthesis.

Two studies have appeared on the latent time of touch sensations. Fröhlich (**20**) stimulates the touch organs electrically, and finds that the time which elapses before the appearance of the sensation varies from 35σ to 160σ, being inversely proportional to the logarithm of the intensity of the stimulus. Stone (**55**) works with a form of the complication experiment in which one stimulus is tactual and the other auditory. It is found that the latent time for the sensation to which the subject is predisposed is less than that of the other, the difference in latent times being of the order of 50σ.

The first of Hahn's contributions (**26**) has to do with the adequate stimulus for temperature. The Weber and Hering theories are carefully considered and criticized. The author formulates his own problem as follows: What is the smallest amount of temperature change which will give rise to a temperature sensation, and does this value depend upon the temperature of the skin itself? The method used was to adapt a part of the skin to a certain temperature by placing upon it a Thunberg temperator for three minutes. The area, thus adapted, was then given a different temperature stimulus by means of the same apparatus. The author finds that warmth and cold sensations do not depend upon the amount or rapidity of change of skin temperature, but rather upon the temperature of the stimulus. This result runs directly counter to the Weber theory, though it is more in harmony with Hering's theory. The paradoxical cold sensation naturally requires a special explanation, and the author regards it as the result of a change in skin temperature so rapid that the usual responses cannot take place. Adaptation, the author regards as a functional nervous condition, and not a physical one depending upon the warmth of the skin. In his second article (**27**) Hahn reports on experiments to determine the depth at which the temperature end-organs lie. On the basis of these experiments he calculates, using Pütter's estimate of the conductivity of the skin, that the receptors for both warmth and cold lie at a depth of about 0.07 mm. Barium sulphide was used to destroy the sensitivity of the epidermis without affecting the cutis. When this was done temperature sensibility was greatly affected, confirming the assumption that the temperature end-organs lie very near the surface. Further experiments were made to determine whether temperature sensations are affected by blood temperature. No such relationship is found. Some experiments were carried out on cold blooded animals and the presence of a temperature sense in these organisms was demonstrated. Bazett (**4**) describes experiments on temperature sensations in which

the temperature of the tissues is recorded by a needle thermo-couple. On the basis of experiments carried out with the needles 2 mm. below the surface, the author concludes that the cold end-organs lie about 0.5 mm. and those for warmth about 1.0 mm. below the surface. This author (3) criticizes Pütter, whose calculations located the temperature end-organs only 0.1 mm. below the surface. According to Pütter's estimate it would take 130 sec. for the maximum velocity of temperature alteration to be attained at a depth of 1 mm., but actually observed values are only about 1/100 of this. With McGlone, Bazett describes in full the thermo-electric apparatus with which their experiments have been carried out (5), reports a study of temperature gradients in the skin and deeper-lying tissues (6), and a further study of the temperature of the air immediately surrounding the skin (37).

Burnett and Dallenbach (9) make a thorough study of the experience of heat. They use a copper heat-grill, combining with each other various intensities of warmth and cold. It was definitely established that warmth which did not itself give heat would do so when combined with cold. The intensity of the heat experience was found to depend upon the temperature difference between the two stimuli of warmth and cold. The limen for temperature-pain was found to vary widely between the observers. The quality of heat was found to lie in the pressure-prick-pain continuum, near prick and closer to pressure than to pain. Dallenbach (12) reports a study of a case of synaesthesia in which stimulation of cold spots with cold stimuli sometimes gave rise to sensations localized in and around the teeth. Cold spots of this kind were found in all six of the body areas studied. A second mapping, made 24 hours after the first, verified 60 per cent of the ordinary cold spots and 81 per cent of the "toothy" cold spots. The secondary experiences were described as light, transient, and pressury in quality. Since the case in question has a synaesthetic inheritance, the author regards these observations as a support for the hereditary theory of synaesthesia.

Hummel (31) studies the phenomenon of temperature adaptation by determining the difference in sensitivity between an adapted area and a neighboring unadapted region. The results are presented in graphic form, and the curves show the superposition of two components in adaptation. These the author regards as being physical and physiological, respectively. The first is the revaluation of the stimulus by the warming or cooling of the skin. The second is a reduction of the excitability of the whole temperature organ in both

cooling and warming. Myers (39) points out that adaptation is not a simple case of fatigue, and criticizes Hering's theory on the ground that it makes difficult the distinction between these two states. Myers himself regards adaptation as the result of a process superimposed by a specific mechanism which brings about a temporary fixed "posture," involving synchronous balancing activities in two antagonistic mechanisms. In a study of thermal discrimination and Weber's law, Culler (10, 11) finds the absolute limens for temperature to be very small. At 28° C. they range from 0.03° to 0.05°, and between 18° and 44° they do not exceed 0.08°. The Weber ratio for the absolute limen of temperature is the lowest ever reported for any sense. It is 0.4 per cent, as compared with 1.0 per cent for vision, 4.0 per cent for hearing, and 25 per cent for pressure. Weber's law is found to hold only for absolute limens, and not at all for differential sensitivity to which it has been applied traditionally. The two limens, absolute and differential, are found to bear an inverse relation to each other. The author develops a theory of sensory experience which regards it as an indication that we are for the time being ill-adapted to our situation. Experience is largely composed of adjustive processes of this sort. The limen reveals how much a given situation must be changed in order to inaugurate a new effort of accommodation by the responsive mechanism.

Schriever (52) finds that the area of a pain stimulus is a very important factor in determining the intensity of the sensation aroused. Both mechanical and faradic stimuli are used. In the application of the stimulus the force necessary to arouse pain increases with increase of area, though much more slowly. Different bodily regions vary from each other in their sensitivity to point stimulation and areal stimulation. The author thinks the results indicate that a distinction must be made between epidermis pain and cutis pain. With electro-osmotic anaesthetization of the skin the pain thresholds for small surface stimuli are found to be increased much more than those for areal stimuli. The author advocates the use of both point and areal stimuli in studies of cutaneous pain. Another study by Schriever (51) is devoted to the analysis of a cutaneous quality described as warmth-pain. Forty areas on the skin and ten on the mucous membrane were investigated with point and areal stimuli. A distinction is made between superficial and deep warmth-pain. Qualitatively these are not found to be essentially different, but deep warmth-pain disappears more readily in adaptation. The article by Davis (14) is a fanciful

description of the evolution of pain and of its present and probable future rôle in the life of man.

Several studies have appeared dealing with parts of the body which show an unusual or anomalous cutaneous sensitivity. Kiesow (34) returns to the study of the cheek area which is usually associated with his name. Hahn and Hajen reported in 1924 that they had found this area sensitive to pain. Kiesow's further study of the area shows it to be lacking in pain sensitivity, a condition which he believes can be explained only on the ground that there are specific nerve-endings for pain and that these are lacking in the mucous membrane of the inside of the cheek. He explains the findings of Hahn and Hajen on the ground that their stimuli affected deeper-lying structures. Kiesow (33) also reports a study of the sensory qualities which result from the stimulation of the uvula. He finds that the lower part of the uvula is sensitive to but one form of cutaneous stimulus, namely, cold. The upper part is sensitive to cold, pain, and touch, but not to warmth. Schriever and Strughold (53) make a study of the sensory qualities of the mucous membrane of the nose and throat. The results are presented in terms of thresholds for the different regions studied. As far as the uvula is concerned, these investigators get the same results that Kiesow did, except that they find the lower part of it insensitive even to cold. The throat in general is found to have high thresholds for all the cutaneous qualities. Pressure is quite lacking, except at the base of the uvula and on the curves of the gums. The authors find the entrance to the nose very ticklish, but the mucous membrane 2 cm. from the entrance does not give touch sensations when stimulated. The cold and warmth senses are present near the entrance, but beyond the entrance thermal sensitivity rapidly decreases and disappears entirely at a distance of 2 cm. from the entrance. Hauer (29) makes a thorough topographical study of the cold sensitivity of the genitalia. Different regions are found to be sensitive to cold in varying degrees, and the distribution of sensitivity corresponds to the distribution of the end-bulbs of Krause, giving further support to the view that these structures are the receptors for cold. Strughold, whose recent work on the cold sensitivity of the surface of the eye gives strong support to the view just mentioned, now contributes a study of the effect of applying warm stimuli to the eye (57). All accessible parts of the eye are studied, and only the edges of the lids and the *caruncula lacrimalis* are found sensitive to warmth. The paradoxical cold sensation was found in all regions sensitive to cold. von Frey and Strughold (19)

investigate the eye-ball and its surrounding parts for their possible sensitivity to touch. They confirm earlier results as far as the cornea and sclerotic are concerned. These surfaces respond to touch stimuli with the quality of pain alone. Certain of the surrounding parts: the skin of the eye-lid, the edge of the lid, the *caruncula lacrimalis* respond with touch sensations and often with tickle, but if the stimulus is increased in intensity they soon respond with pain.

Kietzmann (**35**) makes a study of certain phases of the sense of vibration. He finds that when two such sensations are aroused the stronger tends to suppress the weaker, and the weaker may have the effect of making the stronger more intense. Two stimuli, each alone too weak to arouse a sensation, may give rise to a sensation if they are applied together. The experimenter prepared a large number of vibration stimuli of different types, and found that for the most part these could be readily distinguished from each other by the observers, a result which is regarded as indicating a considerable qualitative richness in this form of sensation. von Frey's article on the sense of vibration (**17**), has to do with Katz's conclusion that the vibration sense is independent of the sense of pressure. The chief support for this view is the result obtained in an experiment by Katz and Noldt.[1] von Frey makes an analysis of this experiment and the conclusion based upon it, and gives his reasons for holding that it does not prove that sensations of vibration may be aroused independent of the touch sense.

Several investigations have been concerned with electrical phenomena of the skin. Rein (**48**) studies the penetration of the skin by coloring substances in an electric field. The penetration takes place through electro-osmosis, for it increases as conditions favor this process. The same investigator further finds (**47**) that certain electric phenomena are excellent indicators of various functional conditions of the sense-organs. Darrow (**13**) experiments with the psycho-galvanic reflex, studying the reasons for the occurrence of this phenomenon as a consequence of bodily excitement. An apparatus is described which secures continuous records of moisture changes in the skin. By this means it was found that there is a correlation between changes in the galvanic skin reflex and moisture changes. The moisture, however, is not the sole cause of lessened resistance, which the author thinks is a function of the break-down of the semi-permeable membranes of the body. Certain sensory changes followed the giving of the stimulus for the galvanic skin reflex. These are

[1] KATZ, D. u. NOLDT, F. *Zsch. f. Psychol.*, 1926, **99**, 104. This article has not been accessible to the reviewer.

described as "tingling, warmth, shiver, swelling," etc. The author thinks that these sensory processes are characteristic of emotion in general, and indicate the need of further study.

Gault (22, 23, 24, 25) reports further progress in his work on the interpretation of spoken language by touch. Some new apparatus has been devised, and the work has been extended to deaf as well as hearing subjects. The author reports the results of experiments (21) which show that vibration rates may be distinguished tactually up to from 2,000 to 3,000 per second. Binns (8) reports some experiments, carried out in England in connection with the wool trade, on the discrimination of wool fabrics, especially by means of the sense of touch. Woodworth (62) contributes to the International Critical Tables some data based upon work in the cutaneous and kinaesthetic fields.

In the field of the kinaesthetic sense, Renquist (49) has made threshold determinations for flexing the index finger. He finds that in order that resistance to this movement may be noticed at all it must have a magnitude of 50 g/cm.^{2} This threshold is not changed if the resistance is exerted with gradually increasing force, instead of being presented with maximum force from the start. This fact, the author believes, marks a fundamental distinction between the strength sense and the sense of pressure. du Mesnil de Rochemont (38) is led, through the study of a pathological case, to make an investigation of the muscle sense with normal subjects. An apparatus something like a Mosso ergograph is used. The subjects work against varying resistances, and yet their reproduction of previous extents is very accurate. When the skin is anaesthetized the same results are obtained. The author concludes that there must be some component in the perception of movement beside strain sensation and cutaneous sensation. What this component may be is considered in another article by von Frey (18). After eliminating other possibilities, von Frey attributes the ability in question to sensations caused by the change in form of the muscle. These may not be regarded as sensations of strain, for strain and the form of the muscle do not stand in an unequivocal relationship to each other. In the supporting tissue which surrounds muscle and tendon, and which to some extent penetrates them, there are a number of nerve-endings quite different in kind from those in the muscle spindles. These von Frey regards as possible sense-organs, mediating sensations caused by change in shape of the muscles. The author believes that the phenomena described here may be related to the so-called reflexes of position. He refers to the work of Magnus (36), and suggests that the phenomena under discussion are

the conscious side of the reflexes of position. Stein (**54**) shows that the illusion of bending the arm may be produced by deformation of the skin. He experimented with a patient who had a broken upper arm. If the skin was stretched on one side of the break by means of pieces of adhesive plaster, and drawn together on the other side, an illusion of bending around the false joint resulted. Tuttle (**59**) studies muscular tonus in a subject who is quite normal except that he has an unusually vigorous knee-jerk, and responds with reflexes in both legs when the stimulus is applied to but one. The experimenter finds that changes in the tonus of the leg to which the stimulus is applied are paralleled by corresponding changes in the other leg. Warden and Flynn (**61**) find that the apparent weight of a carton depends somewhat upon its color and on the color arrangement in which the carton appears.

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MIRROR-WRITING

BY EDWARD C. BLOM

Teachers College, Columbia University

The writer made this review of the literature along with some personal investigations to determine if possible the cause or causes for mirror-writing and to learn if there are any constant accompaniments either physiological or psychological. The eighty-one references used in this work occur in either English, French or German. Most of them are in English. They cover the period from 1878 to 1927. While specifically interested in mirror-writing it seemed advisable to consider rather fully the work done on handedness. Pathological, psychological, physiological and educational points of view are represented in the articles referred to in this review.

As ordinarily thought of mirror-writing is characterized by a reversal of the form and arrangement of letters. Its appearance is like that of ordinary writing held before a mirror. As indicated by its name mirror-writing becomes legible when its image is observed in a mirror. In its most complete form all of the letters of a word are reversed; sometimes, however, only single letters or parts of letters are reversed, due primarily to the confusion of the position of objects in space. Mirror-writing may be produced spontaneously and automatically or it may be the result of a conscious attempt to reverse writing.

The production of this kind of writing is facilitated by the operation of symmetrical accompanying movements which make it possible for an untrained hand to produce motions to which the other hand is accustomed. The ability to write centrifugal with the left hand is likely due to the cross-education from the right hand. Interesting evidence regarding the influence of the interaction of the two hands was furnished by Fuller (30) who found that it was easy to write mirror-writing with the right hand when attending to the left hand, which wrote rightward.

Conditions favorable to mirror-writing.—There is considerable diversity of opinion regarding the conditions favorable to mirror-writing. Hale (37) believes that nearly every child at some period of its development will produce fragmentary mirror-writing with the right hand. It is quite common for children learning to write to

reverse some letters or to turn them upside down. Baldwin (5) found similar reversals in the left-handed writing of adults who usually write with the right hand. Acker (1) is of the opinion that proficiency in mirror-writing can be acquired by any one. A similar point of view is held by Allen (3) who thinks that this ability may be possessed by all persons, but that it remains unobserved. The latent ability may be made evident by a lesion suddenly incapacitating the right arm. Numerous cases manifesting mirror-writing have been observed to follow right-sided hemiplegia (82).

There have been cases reported where defective vision was associated with mirror-writing. In one case after removal of the reflex irritation the writing became at once rightwards, but if the glasses were removed mirror-writing again resulted (74). Hollingworth (38) believes that it is possible that left-handed children who become mirror-writers are usually deficient in visual perception of letters or words, or generally deficient. There are, however, many bright children who form this habit.

Leichtenstern (52) contends that it is not in itself a sign of mental defect. He also believes that it rarely indicates nervous disease. According to Tuke (77) mirror-writing is found under many conditions such as forms of mental weakness, hysteria, and in cases of moral perversion. A neurotic inheritance may encourage it. He also observed that it is more common among women than among men and that it is easily acquired by highly nervous people. Hale (37) finds that as a pathological condition it is very frequently found in children with impaired intelligence, deaf-mutes, the blind and cases of katatonia. While Buchwald (14) found it one of the first manifestations of aphasia Bastian (9) is of the opinion that it does not have any special connection with aphasia. According to Peckham (63) mirror-writing may be a sequela of weakness by disease, of weak-mindedness in children, of left-handedness, or merely absent-mindedness in normal persons.

A large number of observers associate mirror-writing with handedness. Acker (1) believes that it occurs spontaneously only in left-handed children, or adults after right-handed paralysis. Beeley (10) says, "All of the mirror-writers write mirror-wise with the left hand." Buchannan (13) speaks of it as a congenital tendency, almost a defect in left-handed children. Although many believe that mirror-writing is only of pathological significance more place emphasis upon it being the normal way for the left hand to write.

Theories to explain mirror-writing.—Fuller (30) gives the following classification of the theories to explain mirror-writing:

1. The explanation which depends upon the facility of external motions of the limbs.
   Advanced by Durand, 1881–1882; Peckham, 1886; Clapham, 1894–1895; List, 1901; Wilkes, 1902; Wray, 1903.
2. Those which place the emphasis upon the facility of centrifugal motions of the limbs, but in addition attribute the ultimate causation of specific movements to bilateral representation on the cerebral cortices.
   Advanced by Erlenmeyer, 1879; Ireland, 1881–1893; Leichtenstern, 1892; Acker, 1894; Mills, 1894; Kingman, 1905; Buchannan, 1908.
3. Those hypotheses based primarily upon bilateral representation on the cerebral cortices. This group is closely allied to group two.
   Advanced by Peretti, 1882; Bianchi, 1883; Bruce, 1895; Auden, 1909; Burr and Crow, 1913.
4. Those that admit but a single writing center.
   Advanced by Smith, 1879; Allen, 1896; Elder, 1897; Russell, 1900; Jones, 1903.
5. Those that depend upon disturbance of vision or of visual center.
   Advanced by Sweeney, 1900; Hale and Kuh, 1901; Pendred, 1908.
6. Those which recognize various controlling factors for individual voluntary reversals. Thus the movements may depend upon motor, visual, mental or auditory-motor imagery.
   Advanced by Abt, 1901; Downey, 1908.

The frequency of occurrence of mirror-writing.—By collecting information from the teachers in the Chicago schools Beeley (10) found one mirror-writer to every two thousand five hundred children. As the result of actual tests Gordon (33) found one-half of 1 per cent among normal children, and 8½ per cent among feebleminded children. Gordon's results certainly do indicate that mirror-writing is much more common among feebleminded children than among normal children, but they do not prove that mirror-writing is an indication of feeblemindedness.

Experimental findings of Fuller (30).—In order to get more complete information on mirror-writing under controlled conditions Fuller carried out a large number of experiments, the summary of which follows:

Hypnosis, four subjects. The subjects were normal but were tested in deeper stages of hypnotic sleep. All could be made to do mirror-writing with the left hand. Three immediately and one after being carried through successive stages of distraction of attention from the left hand, and of dissociation of the mental from the motor functions.

Hysteria, one subject. In these subjects there was complete anesthesia of the left side of the body. The left arm at first produced no result, but after suggesting that it was the right arm on the planchette mirror-writing was obtained with the left hand although the subject was unaware of the movement of the arm.

Drugs, twenty-five subjects. Eighteen of these subjects were under the influence of alcohol. They were tested as found. Most of the cases produced mirror-writing with the left hand especially when forced to write quickly. In general the more intoxicated they were the more likely they were to write reverse. Suggestions resulted in more mirror-writing. Three subjects were tested to determine the effect of cannabis indica. With these right-hand writing was tried before the drug was taken. There was no sign of reversing. In two cases the left-hand writing was mirror fashion in most trials. The other case produced only 3 per cent, but hesitation and confusion were great. Suggestion again gave positive results. Four subjects were tested while under the influence of ether. These tests were made during recovery of patient, but before consciousness. One made no reversals. Three made reversals. Suggestions produced positive results with two patients.

Abstraction, fifteen subjects. These persons were in normal life. The abstraction was produced by mental arithmetic, crystal gazing, etc. Here the first strokes with the left hand were mirror-wise 30 per cent of the time.

Insane, five patients. These patients were confined in an institution and were in such a condition that there was no hope of recovery. One patient looked at the examiner and wrote mirror-writing with the left hand every time, the other four about 16 per cent of the trials.

Hemiplegia, right side paralyzed, two insane patients. The one whose mental condition was very poor wrote in reverse direction every time, the other about 9 per cent of the trials. There was little hesitation.

Feebleminded, sixty-nine patients. Those in the lower grades reversed or were disturbed most with their left hand. The patients of marked neurotic history did more mirror-writing than those who were only feebleminded. All of them could copy the words when written in reverse fashion.

Deaf and dumb, seventy-seven patients. Only two wrote mirror-wise with the left hand. When the paper was turned through 180

degrees between the writing of a double word, twelve more reversed the second part of the word.

Blind, five subjects. None of these subjects were congenitally blind. None of them wrote mirror-fashion with the left hand, and none could be readily taught to reverse their script.

Normal children, twenty-six subjects. Eighteen children who had just learned their letters made no reversals by ordinary means except in confusing letters like S and N. Eight children one grade further on frequently reversed confusing letters with the left hand and they could be confused considerably.

Persons with special training of the arms. Two groups were used for this study. Twelve clerks were in the one group. When writing with both hands at the same time the left hand wrote always rightward. After these people had been told how to write mirror-fashion and when both hands were active with little attention to the left hand, it was simpler for them to write reversed with the left hand. Twelve plasterers constituted the other group. When they used both hands at the same time the left hand gave a correct imitation of the production by the right hand. It was much harder for these subjects to make drawings in one direction with one hand and in the other direction with the other.

University students, thirty-one subjects. Writing simultaneously with both hands they produced no mirror-writing with the left hand. With eyes closed and writing rapidly with both hands six showed a tendency to reverse the left-hand figures. All reversed a few lines, generally the first strokes. Where they were in a condiiton of abstraction the effect was more prominent. After they were given an explanation of the process all of them found it easy to do mirror-writing, particularly when the right hand was passive. They found it easier to reverse with the right hand, each being used separately. Moreover, when both hands were active the mirror-written left-hand figures were the more perfect. After having practice it was easy for them to do mirror-writing with the right hand while attending to the left.

Reversed visual field, sixteen subjects. The field was reversed by using a right-angled prism. There were no reversals with the right hand, but there was some confusion. After the attention of the subjects was called to the visual appearance of the writing there was little effect on the right hand, however, with the left hand the confusion was raised to 90 per cent. Further results using reversed visual field

are given in Table I. Here the method used in memorizing the figures is given under "Series." The sense which the reagent was told to rely upon is headed "Control." The first column of figures is the number of reagents, the second column is the average number of reversals in every twenty trials.

Most reagents reversed very little. With the right hand reversals were extremely rare; when they did occur they persisted after the prism was removed, indicating errors of memory. The left-handed

TABLE I

Showing Effect of Method of Learning on the Number of Reversals
(Taken from Fuller's Data [30])

Series	Control	Right Hand		Left Hand	
		Reagents	Reversals	Reagents	Reversals
Memory by eye	General	1	0	10	0
		10	12	1	16
		2	4	3	12
	Visual	8	0	3	0
		1	20	4	20
		2	17	3	14
		2	12	3	12
		1	7	1	2
	Muscular			3	0
		13	0	2	8
		1	4	9	4
Memory by right hand	General	13	0	8	0
		1	6	3	12
				3	8
	Visual	12	0	3	0
		1	16	9	6
		1	7	2	4
	Muscular	13	0	4	0
		1	8	3	12
				6	8
				1	4
Memory by left hand	General	12	0	11	0
		2	4	2	8
				1	4
	Visual	4	0	2	0
		7	10	1	20
		2	8	9	8
		1	4	2	4
	Muscular	11	0	11	0
		1	16	1	8
		2	10	2	4

reversals were more frequent and did not show corresponding tendency to persist after removal of the prism. Although vision is a most important guide to motor expression of the psychic areas these results show that the motor memory is the more important factor and that muscular action is in imitation of the direction of the thought.

Hollingworth's ideas regarding correction of mirror-writing.—Hollingworth (**38**) says:

"In order to correct the difficulty, visual control of movement must be cultivated. Attempts to correct by changing over to the right hand are injudicious for the reasons[1] cited under the discussion of left-handedness.

"In securing the control of visual perception and imagery, it is well to have the child write slowly and carefully from a copy, not being allowed for some time to write spontaneously. At first, particularly, the teacher may guide the child's hands and urge him to notice in detail how another writes. Of course, the best educational treatment is that which never permits the development of the habit in the first place. This could be accomplished by careful watching of all left-handed children, at the very beginning of their attempts to learn to write. As each letter and figure is taught for the first time, the child whose natural impulse is to reverse it could be made conscious of his error, and could be drilled in the coördinations of hand and eye which produce the correct response. In the very large beginners' classes which are customary in the public schools, such careful attention to the needs of individuals is here, as in other respects difficult to give."

Theories advanced regarding handedness.—Further consideration of handedness is given here because of its apparent importance in connection with mirror-writing. "Left-handedness," by B. S. Parsons, gives a splendid bibliography on this subject (**63**). In this book Parsons classifies the theories advanced to account for handedness as follows: (a) Habit, (b) Hereditary transmission, (c) Nursing and early education, (d) Visual distribution and displacements of center of gravity, (e) Primitive warfare, (f) Inequality of blood supply of the brain, (g) Origin of subclavian arteries, (h) Superior development of one cerebral hemisphere, (i) Ocular dominance. His own ideas regarding handedness are given in the following summary:

"That earliest man, like the highest apes, possessed pure binocular vision. He sighted laterally, with either eye as needed, and was, therefore, like the simians, ambidextrous.

"That the fixed unilateral sighting faculty, accompanied by handedness, developed with the manufacture and intelligent use of weapons, and that in the beginning it was without any marked general bias for either the right or left side.

[1] These reasons are given later in this paper under *Theories advanced regarding handedness.*

"That the biological ascendancy of right-eyedness and right-handedness subsequently came about through natural selection as a result of one or more now obscure causes, the most likely being the very obvious advantage that would accrue to the warrior who as he faced his opponent carried his spear or club in his right hand. With the invention of the shield this initial dextroid tendency would of course be intensified. There is also a strong probability that sun-worship and its derivitive superstitions had much to do with fixing man's dextrality, which would be further strengthened by finger counting and gesture language.

"We are thus brought to the conclusion that eyedness, accompanied by handedness, is a fixed characteristic of the race, pure binocular vision accompanied by ambidexterity representing the ancient parent type, and right-eyedness accompanied by left-handedness the two subsequent mutations."

Parsons believes that changes in handedness are always deliberately brought about while changes in eyedness are involuntary.

Left-handedness is not at all uncommon. Hollingworth (**38**) says that 4 per cent of the population are left-handed. She believes that there are many degrees of left-handedness and that so far it cannot be said to correlate with organic inferiority. She cites Ramaley's study of 610 parents and 1,130 children, in which he arrived at the conclusion that left-handedness is inherited (as a Mendelian recessive) and is potential in about one-sixth of the population.

Tests for handedness.—Because of his ideas regarding the association of eyedness with handedness Parsons (**63**) uses for his tests of handedness an instrument which he calls a manuscope, its function being to determine which visual line is used for sighting. Hollingworth (**38**) lists the following as the best tests for congenital left-handedness, tapping with the wrist movement, tapping with the fingers, spontaneous rubbing, throwing and picking up, winding, and cutting with a scissors.

Correction of handedness.—Twitmyer (**62**) says:

"In my opinion it is impossible to change a congenitally left-handed child into a right-handed child. There is no evidence on record that this has ever been accomplished. It is possible to train left-handed children to many finely coördinated movements, such as writing with the right hand. From my point of view this is desirable and renders the child less awkward in an environment constructed for right-handed people. Under ordinary circumstances no damage is done the child by such training."

Hollingworth (**38**) believes that persons strongly left-handed should not change over because of the possibility of nervousness and speech defects.

It is interesting to notice how proficient left-handed people become

with both hands apparently without much effort. Acker (1) describes a case where a boy broken of left-handedness could write equally well mirror-writing with either hand and rightward writing with either hand. Fuller (30), working with two cases, found similar results. The writer has also observed similar cases.

Relation of left-handedness and mirror-writing to reading and spelling difficulties.—Orton (61) believes there is a relationship between mirror-writing and reading difficulties such as the tendency toward word reversal. Dearborn (16) believes that just as the left-handed person may find it easier to write from the center of the body toward the left, so he may be able to read more readily from right to left. This might account for confusion of letters that are the same in form but different in position such as, p, g; d, b. Left-handed persons have a tendency to catch the last letter of a word first. This kind of eye movement could easily lead to reading and spelling difficulty. Dearborn and Carmichael believe that the ease with which work can be seen is an important factor in determining the natural tendencies in reading and writing.

According to Gates (31) extreme left-handedness might lead to wrong habit formation in learning to read. He suggests that inquiry concerning handedness be made in cases when there is extreme difficulty in word recognition. He writes about a left-handed pupil who was occasionally subject to mirror-writing and had trouble in reading and spelling. With this pupil oral reading difficulties nearly always enhanged speech difficulties because of wild guessing at words. With regard to helping left-handed people who develop this difficulty Gates writes:

> "The similarities and differences, common elements and contrast words need to be emphasized; the means of attacking words in a straightforward manner, breaking them into units, reacting to the units singly or in combination as the case demands should be emphasized."

He believes that if the case is very stubborn careful writing of words under guidance should be resorted to.

The writer has had the opportunity to observe very closely a young boy who has a tendency toward mirror-writing with his right hand. This particular boy is above normal according to intelligence tests. He is unquestionably left-handed. He manifests no speech difficulties. He does have a tendency to read numbers backwards when he is not careful. He was taught to print with his right hand and does very well. He does mirror-writing only when he is careless.

The above facts seem to indicate that there is a definite relationship between reading troubles and mirror-writing in this case at least.

In order to learn if there is any general tendency among left-handed adults to read from right to left the writer asked fifteen left-handed people if they had to guard themselves against this error when reading many numbers. All but two of them said that they had noticed trouble in this respect. The number of cases considered here is too small to be of real value in drawing any conclusions.

Conclusions.—A review of the above literature has led the writer to believe, (1) that mirror-writing is as normal for left-handed people as the conventional writing is for right-handed people, (2) that a child who does mirror-writing is likely to have reading and spelling difficulties, (3) that mirror-writing is not an indication of inferior intelligence, (4) that except in rare cases a left-handed child can be taught to write with the right hand without any serious consequences, (5) that should a change be necessary the left-handed child could soon learn to write with his left hand as well or better than he did with his right hand.

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THE RÔLE OF SPEED IN MENTAL ABILITY

ROSS A. McFARLAND
Columbia University

In this review an attempt will be made to classify and analyze the various types of researches related to the rôle of speed in mental ability. The types of studies which have been selected may be divided in a general way as follows: (a) those investigations in which the speed of reaction has been studied in connection with sensation and perception or in the simpler psychological processes; (b) those investigations in which speed of reaction has been related to the higher mental processes or some factor in mental ability such as attention, imagination or memory; and (c) those investigations dealing with rate of response in connection with general intelligence or with a specific ability. However, no strict lines between these three general types of researches can be drawn, and on account of the overlapping of the subject matter it is impossible to present the studies according to the above classification. The conclusions of many of the studies are so indefinite and general as to prevent presentation according to evidence for or against general ability in respect to speed. Therefore the studies which have been selected for review will be presented in chronological order.

The earliest attempts of Binet (**28**) in studying intelligence consisted in measuring factors of sensory discrimination, reaction times and other factors in which rate of response was an important element. The investigations of many of Binet's contemporaries (**28**) showed this same general tendency. In 1896 Binet and Henri in an article on "Individual Psychology" (**5**) discussed the question of the inter-relationships of different mental functions and stated that persons who are slow in ordinary reactions, such as walking or writing, are probably slow in reaction experiments. Later Binet qualified this statement by saying that this is only a probable conclusion. In 1900 Binet (**6**) concluded that speed in his tests was not related to the intelligence of the children whom he selected by other means as bright and dull. In 1905 and in 1908, when he devised his tests, he paid little attention to the amount of work that could be done in a given length of time. The Stanford Revision followed Binet's sug-

gestion, for only seventeen of the ninety tests composing the scale have a time limit. It should be noted that sufficient time was allowed to complete the test, but no reward was given for finishing early. It is fair to say, however, that most group tests adhere rigidly to a time limit which indicates the importance psychologists have attributed to the time factor since Binet's experiments.

J. A. Gilbert's study (15) is among the first reported in this country, and it shifts from the previous emphasis upon sensory reaction time toward the consideration of speed of reaction in relation to mental ability as displayed in school work. In this study an extensive investigation was undertaken with one hundred school children from ages six to seventeen. The part of the study which is relevant here is the data collected on simple visual reactions and discriminating visual reactions as shown in laboratory tests of ability to recognize colors. Gilbert found that the bright children reacted much more quickly with discrimination and choice than the dull. He concluded that "a child's mental ability could be judged by the quickness or the rapidity with which it is able to react." The procedure and the conditions under which these experiments were given allowed ample room for irregularities and discrepancies to distort the data. The only criterion for "brightness" was the judgment of the classroom teacher, and quickness entered into the teacher's judgment of the pupil's "brightness." Hunsicker (21) has pointed out quite correctly that children ranked for "brightness" (on the basis of quickness) would naturally show a reaction time which has a fairly high correlation with "brightness" in laboratory tests.

A study of the same general type as the above is reported by Bagley (3) in 1901. He carried on an extensive investigation by giving to one hundred and sixty school children tests for motor ability, and to one hundred and seventy-five school children tests for mental ability. Scores in these tests were correlated with the mental ability of each child as determined by a class standing and by the teacher's estimate independent of class records. Bagley concluded from the data that there was a general inverse relation existing between motor and mental ability, *i.e.*, those who are brighter and have quicker reaction times are, as a rule, deficient in motor ability. Bagley upheld Gilbert's assertion (15) that "brightness" is directly related to reaction time. Hunsicker (21) has shown, however, that this conclusion is based upon the use of two different names for the same score and it is evident that a relationship between two measures of mental ability is not found. The same general criticisms concern-

ing loose procedure and faulty interpretation of data which were directed toward Gilbert may also be applied to Bagley.

In 1901 Wissler (34) reported a study conducted over a four year period with sixty college freshmen. A series of mental and physical tests were given; the ones of chief interest to us are reaction time, rate of perception, and rate and accuracy of movement. He found a chance relation between class standing and tests of quickness but the conclusions in general are in opposition to those reached by Gilbert and Bagley that quickness and ability are related. Wissler says that reaction time is useless as an indication of mental ability or of general alertness of thought and action. His study has been severely criticized because of the laxity of experimental procedure, and much doubt has been cast upon his broad assertions and conclusions. For example, in the reaction time experiment Wissler obtained the data by taking the average of from three to five "valid reactions recorded by the observer." One would also question his use of class standing in the case of college students as a reliable indication of mental ability.

Aikins, Thorndike and Hubbell (2) published an article in 1902 dealing with the relationships of a number of perceptive and associative processes, all depending on quickness and accuracy. One hundred and sixty boys and girls in the eighth grade and eighty in the fifth grade were used as subjects in tests of marking misspelled words, checking letters "r" and "e" in a passage, hard and easy opposites, alphabet test and simple addition. There was an average correlation of .51 between the amount done in the addition test and the amount done in the three combined association tests. The intercorrelations varied from 0 up to as high as .60; the average was approximately .40. The investigators say of these results, "Quickness of association as an ability determining the speed of all one's associations is a myth. It has been the habit of psychologists to use the words 'memory,' 'attention,' 'delicacy of discrimination,' etc., as if they referred to general mental functions and the words 'quickness' and 'accuracy' and 'ability' as if they referred to general mental qualities. But any consideration of the potent facts of human nature suggests that *a priori* it is more rational to look on the mind as a multitude of particular capacities, particular associations and particular acts, all of which may be highly independent of each other."

In 1904 Whipple (33) reported a study of reaction times as an indication of mental ability. In analyzing the reaction time experiment he pointed out the variability of results which may be obtained

due to such factors as lack of constancy in giving the stimulus and various subjective factors which are difficult to control. A careful criticism and analysis is made of the studies (reported above) of Bagley (3), Gilbert (15) and Wissler (34), and Whipple upholds the criticisms that have been given as to the laxity of procedure and the small number of reaction times recorded. He asserts that the reaction time experiment is no indication of mental ability and that when one obtains positive results with school children faulty methods are apt to be the cause. He says that the outcome of reaction time experiments will depend largely upon the attention of the observers and upon the instructions given.

A study in which sensations furnish the field of experimentation was reported by Henmon(18) in 1906. The findings of the earlier researches of Weber, Fechner, Müller, Wundt and Cattell were utilized. By varying the intensity of the stimuli he studied the length of reaction time as a measure of the difference in the sense discrimination of two subjects in three sense departments—color, pitch and linear magnitudes. A Hipp chronoscope was used to record the time of discrimination. After analyzing 4,400 reactions he concludes that, at least for the two observers used, a measure of the reaction time is a measure of the ability in that sense category. Henmon believes that the unit of measurement derived from a comparison and evaluation of reaction times in the various sense departments based upon the amount of stimulus necessary to produce each, gives rise to a common unit of measure in terms of reaction time. This unit of measure he asserts is applicable for measuring the amount of sense ability in the various sense departments investigated. This study is significant in suggesting a method for the measurement of the higher mental processes by reaction times and in demonstrating the value of extensive and precise work with a limited number of subjects. In a more recent and indirectly related study Henmon(19) describes a series of experiments with lines, the object of which was to find the relation of the time of judgment to its accuracy, and to study individual differences in judgment times. Three subjects were used and 1,000 reactions were taken for each subject. Henmon observed in this experiment that the average times of correct judgments were shorter than for wrong judgments, and that there were marked individual differences among his subjects in accuracy, time of perception and degree of confidence.

In 1909, Burt (8) reported an experiment in which he found correlations between speed in various performances and intelligence

in the case of thirty elementary school children. The correlation between intelligence and card sorting gave .52 and the correlation between intelligence and speed in alphabet sorting gave .61. The intelligence of the boys was estimated by the headmaster. These tests involved finger dexterity to such an extent that mental quickness was not fairly tested. Furthermore, the basis for judging the intelligence of the boys may be seriously questioned from the standpoint of careful laboratory experimentation. The same criticism which we have applied to Burt may also be applied to the experiment reported by Brown (7) in 1910. Brown compared the intelligence of 39 girls and of 40 boys as determined from the independent ratings of two teachers, with ability in respect to speed. The measure of speed was the number of letters e-r and the letters a-n-o-s which his subjects could cross out in five minutes. The tests were repeated at the end of two weeks. The correlations were largely negative.

In an attempt to ascertain the correlation of different tests with the subjective estimate of intelligence and to select the tests which give the highest coefficient of correlation Wyatt (35) gave a series of tests ranging from checking the letters e-r to interpreting fables. Thirty-four children were used as subjects. Wyatt used an arbitrary system of penalizing errors and obtained correlations of .40 and .45 between quickness in the cancelling tests e-r and a-n-o-s, respectively, and intelligence.

In the work of Hart and Spearman (17) on dementia four types of tests were used, one test of sensory perception, one of quick and accurate usage of well worn associative paths, one of a more elaborately synthetic character, and one depending on the wealth and discrimination of ideas. Both normal and abnormal subjects were used. One finds the statement in this study that the specific correlation between the speed of different tests is —.09 from which they infer that there is no constancy in the speed of executing different tasks. Hart and Spearman fail to find any evidence for a group factor of speed, *i.e.*, one common to a number of specific abilities but independent of General Intelligence, the question which primarily interested them. In their study the "specific" correlations express the correlations between two tests when the common dependence on the entire cortex (G) is eliminated by mathematical treatment. Gilbert (16) pointed out that the examination of the gross thing called speed in the performance of mental tasks preliminary to the study of its relationship to intelligence is an entirely different problem. If one finds the correlations between the speed tests in the Hart

and Spearman study before "G" is eliminated, the results fall between .13 and .30. Even these results may be questioned in relation to the constancy of speed, first because the scores are based on speed tests which do not indicate whether speed in time or accuracy in time is represented, and second because, if the material used had been more homogeneous, the constancy in the speed factor might have been much higher. Furthermore, it is doubtful if the rôle of speed can be clearly demonstrated without keeping accuracy constant and without careful laboratory procedure in timing each individual item of a test.

An interesting study was reported by Judd (22) in 1916, part of which bears directly on our topic. He arranged the results of 1,831 children in reading, in the Cleveland study, so as to indicate the relation of speed to quality. Each child was put into one of three groups (rapid or the highest 25 per cent, slow or the lowest 25 per cent, and medium or the middle 50 per cent) according to his rate score. A similar division was made with regard to quality. Judd said, "These findings seem to emphasize the fact that good readers are usually not slow and poor readers are usually not fast. . . . For the purposes of this survey the general fact that high rate and good quality are commonly related and that low rate and poor quality are commonly related is of great importance." King (23) in 1916 published the results of a study relating to speed and quality in reading, using ninety-three university students as subjects. The method employed was different from Judd's. After certain passages were read against time the observer was tested as to what was remembered. Between rate and comprehension he found a correlation of —.07. He concluded that there are all degrees of differences among individuals in rate of reading with no regularity in results in that the quick subjects include persons of various degrees and types such as the keen, the dull, the thorough and the superficial.

In 1916 McCall (25) raised the question of the superiority of "speed" tests to "power" tests as measures of mental ability in relation to correlation, convenience, or to time spent. By a "power" test he meant one that contains units sufficiently difficult to discover the maximal ability of persons tested with a time limit sufficient for each to reach the limit of ability. Eighty-eight boys and girls in grammar schools were used as subjects. Four "speed" and four "power" tests were given. McCall concluded from his investigation that the "power" tests give a much higher correlation with mental ability than do "speed" tests. This study is significant in that it is one of the first to deal with the relative importance of rate and

"power" in the measurement of mental ability. However, the results are by no means final in that the group procedure and the tests used lacked the necessary refinement to test the relative value of "speed" tests and "power" tests. McCall and Ruger (**26**) found virtually the same results as reported above in repeating the experiments a year later.

Anderson's (**1**) study reported in 1917 attempted to determine more precisely for children the rate of mental association in responding to words. No definite correlation between intelligence and speed of association was obtained. Anderson thought the type of answer had some relation to intelligence as rated by class records and teacher's verdict but he concluded there was no correlation between speed of association and intelligence. He stated that more importance should be attached to the kind of answers than to the speed of reaction. The evidence for these assertions is not clear and he asserted in one place that speed in comprehension of an idea may be an index of intelligence.

Mark A. May (**24**) carried on an extensive investigation concerning the rôle of speed in the Army Alpha test during the World War, using 510 recruits as subjects. This study is also reported by Yoakum and Yerkes (**36**). A correlation of .965 was found between scores made with the usual time and scores made when the subjects were allowed to work for another period of the same length. The constancy of the relative positions of the subjects with double the time has been widely quoted and interpreted as evidence for a high degree of relationship between quickness of performance and degree of mental ability. Hunsicker (**21**) has shown that the importance of the results of this study have probably been overestimated since Army Alpha is not a *bona fide* speed test. For limited intellects it tends to be a power test and for superior intellects it is a speed test. Obviously, then, Army Alpha cannot be both a rate test and a power test for all performers.

Gates (**13**) in 1921 reported an investigation in which the rate of reading was related to the comprehension of what was read and to general intelligence. In using a composite of several tests in measuring rate, comprehension and group intelligence, he obtained correlations ranging from .31 for the Stanford M.A. with rate, to .79 for rate with the directions test. The correlation for group intelligence with rate was .64 and the correlation for rate with comprehension was .84. In 1924 the same author (**12**) reported an extensive study of speed and quality of handwriting. He discussed the many obstacles

which have made difficult the measurement of the two traits and he quite correctly recognizes that each must be considered in a judgment of the final written product. He suggested a formula (12) which might be used for the combination of scores in the two traits in order to arrive at a truer measure of handwriting ability. A great deal may be said for a relationship of this sort without commitment to degree or to amount of rate or quality. Courtes (10) also reported a study in 1924 on the speed and quality of handwriting. Like Gates he recognized the numerous complexities which come to the surface in a critical study of this nature. In order to obtain a truer indication of speed and handwriting ability he also suggested a formula (10) derived from his careful experimentation upon school children.

In 1922 Freeman (11) conducted a class study of the relation between speed and quality of work with 114 subjects, largely graduate students. Two tests were given upon the contents of the course—one a multiple answer test and the other a completion test. He was interested in correlating rate, as determined by the order of finishing, with the quality of performance as measured by the tests of the course material. The correlations were interpreted to indicate that there was no relationship between speed and quality for this college group and he concluded that "there may be real differences in the quality of work of which one is capable which are independent of speed of performance." There is a very common mistake made in this experiment which invalidates the conclusions in that the time taken to produce the maximum score on a power test is taken as a measure of rate of work. It is also obvious that many extraneous factors, subjective and objective, enter into the actual time consumed by a college student in an examination.

In 1922 Garrett (14) published the results of his experiments on speed and accuracy in the perception of small differences, using lifted weights, linear magnitudes and handwriting specimens, and on speed and accuracy of coördination, using a thrusting experiment and tracing or image experiment. The conclusions are not clear-cut but it is interesting to note several of his findings: (a) "The loss in accuracy with increased speed is relatively very slight." (b) The experiments on lifted weights, linear magnitudes and handwriting have shown that accuracy is not highest when speed or rate is slowest but that accuracy tends to increase gradually from the slowest rate used. (c) An observer holds his relative position as fast and accurate in experiments where the functions involved are closely related.

A study similar to that undertaken by May (24) was the one

reported by Ruch and Koerth (29) in 1923. The subjects used were college freshmen who had previously been ranked on the basis of Thorndike's, Morgan's, and the Iowa Comprehension tests combined. Seventy students were selected from the lowest deciles and fifty-two from the highest. They were given the Army Alpha "Form 7" so as to reveal their scores for single, double and unlimited time; different colored crayons were used during each period so as to distinguish the progress made during the period. A correlation of .966 was obtained between single and double time and .945 between single and unlimited time. The authors make the following statements in conclusion: (a) "Admitting that Army Alpha is largely a speed test the fact that single time correlates .966 with double time and .945 with unlimited time indicates that the speed factor does not seriously invalidate the test." (b) "Increasing the time allowance does not permit dull subjects to equal the scores of the more intelligent subjects." (c) "The present findings substantiate the important findings of the earlier Army investigation when proper allowance is made for the fact that Alpha is far too easy for good students of the college group." Highsmith (20) has criticized these conclusions on the basis of the fact that the correlations have been obtained from highly selected subjects, *i.e.*, the subjects who were in the median deciles or the central tendency of the original group were left out altogether.

Chapman (9) in 1924 reported an investigation of the relationship between speed of work, persistence and success. Eighth grade pupils were used as subjects in the simple mental task of building as many words as possible with five given letters. The experiment illustrates the rigorous analysis and selection of data which the author states should characterize all investigations of this kind. He finds as a result of his research that there are great individual differences in these three traits, with indications that the fastest workers achieve greater success (but the margin is disappointingly small), and that the fastest workers show less persistence than do the slower ones. He also states that speed and success show a higher correlation when the persistency factor is made constant by requiring all to work for a stipulated length of time.

E. Bernstein (4) at the University of London reported an experiment in 1924 which is related to the problem of a general speed factor in intelligence. He was primarily interested in the same problem as Hart and Spearman (17), namely, whether or not speed is a "group" factor independent of General Ability. He also considered whether speed and intelligence are related. Bernstein attempted to

eliminate General Ability or Intelligence, not by the mathematical procedure of Hart and Spearman, but by giving the subjects two kinds of tests: (a) Leisure Tests or those in which ample time was allowed, and (b) Haste Tests or those in which the time allowed was too brief for any but the fastest subjects. Since the slower subjects could fairly be expected to do relatively better in the Leisure tests than in the Haste tests, a comparison of the performances in these two kinds of tests should therefore be expected to throw light on the nature of "slowness" and its relation to intelligence. E. C. Gilbert (16) has criticized Bernstein's procedure since this measure gives "slowness" only under the assumption that speed and accuracy are inversely correlated. Ten sets of tests were devised embodying this principle and were given to 158 children from twelve to fourteen years of age. In the computation of the results after the correlation of the Leisure and the Haste tests were obtained, the validity of which has been questioned, Bernstein correlated this with the estimates of "slowness," made under the instruction to divorce the concept from intelligence. The reliability of the times is low as a result of the difficulty in making them. Bernstein concluded, that "the low correlation of Leisure and Haste tests as a measure of 'slowness' with the estimates of the same quality is altogether inconsistent with the existence of a general speed ability." It is not clear whether he regards his alternative as established or not, *i.e.*, the identification of speed with intelligence. Anyway, the experimental evidence for it is lacking and the possibility that speed may be a function of the special abilities does not occur to him. The conclusions of this study then may be questioned since the Leisure and Haste tests and the estimates are both of doubtful validity.

In 1925 Highsmith (20) reported an interesting study concerning the relation of the rate of response to intelligence. Eighty-seven boys and girls were used as subjects. Highsmith employed three sets of measures including (a) one set as nearly free from the time factor as possible, the Stanford Binet test, (b) one set including both the time factor and difficulty, the National Intelligence test, and (c) one set as nearly as possible pure speed test, Ordinary Mental tests—both linguistic and non-linguistic. The tests for speed were restricted to common mental tests material so that the same mental functions would be employed in all three sets of tests. The inter-correlations ranged from as low as .33 to as high as .88. Highsmith's conclusions are in substance that the rate of response to test material is not a safe measure of intelligence, that the National Intelligence test is a

better measure of rate of response than of intelligence, that the simple linguistic rate tests is about as good a measure of intelligence as the National Intelligence test. He also says that the high correlations between the rate tests and the National Intelligence test points to the danger in employing the composite group test as a criterion by which the validity of a new group test is tested in that this process may increase the importance of the rate element at the expense of factors which may be more significant to general intelligence. Of Highsmith's investigations one might note that the rate factor comes to the fore as quite an important element, in spite of the fact that the tests were given to children in groups and lacked the refinement of methods necessary to pin down so difficult a factor as speed, that there is a constancy of the speed factor existing in a number of the rate tests as well as a fairly high correlation between the rate tests and the National Intelligence, and that the correlations are not so low as to warrant the conclusions drawn by the author.

An investigation reported by Clark (9) in 1925 relates to the relation of speed, range and level to scores on intelligence tests. This problem was advanced by E. L. Thorndike (31) in accord with his suggestion of analyzing ability into level or power, range of information, and speed of reacting. One hundred and eighty pupils of grades seven and twelve were used as subjects. The Otis Self-Administering Group Test, the Terman Group Test and the Stanford Revision of the Binet Test were used to measure intelligence. The two measures of speed were obtained from the quickness in solving arithmetical problems and completing sentences. The individual method of timing each item was used and the work of pupils under 80 per cent in accuracy was discarded. The correlations of rate with the scores on the intelligence tests averaged .54.

An important contribution to the problem of the relationship between rate and ability was made by Hunsicker (21) in 1925. In this study the author defines more carefully than any previous investigator the rôle of speed in mental ability. The method of procedure was that of individual timing, in some cases of each item of the test and in others of each page of items. The two tests used were arithmetical problems and sentence completions. The subjects who took the tests included two groups of college students, twenty-eight and forty-four, three groups of children. The largest group contained two hundred and five children. In the statistical treatment of the data the observers were divided into ten different levels in accordance with the completion of tasks of known or established difficulty. The level in

each function was taken as the measure of power in that function and an attempt was made to keep accuracy constant by discarding incorrect responses. From the results obtained the author states that for the groups studied there is a consistent and fairly positive relationship between rate and ability. He concludes that individual testing has decided advantages over group testing and is essential for investigations of this type, "that the positive correlations, although not high, are evidence of the constancy of the relationship between rate and ability and that, although the findings lack the necessary finality which the importance of the main issue calls for, they do reveal a relationship as to the general trends and constancy of which there can be no serious doubt."

An even more careful study as to method than that of Hunsicker was reported by Peak and Boring (27) in 1926. The investigation was carried on in the laboratory with five observers, all advanced students in psychology. The authors recognize the ultimate necessity of checking the conclusions of such a study on a larger and more heterogeneous group but they show the advisability of a laboratory study of certain problems related to the nature of intelligence. These subjects were given forms 5 and 6 of the Army Alpha Examination and forms A and B of the Otis Self-Administering Tests. With accuracy kept constant by using only those items correct for all observers, significant individual differences were found to exist between the subjects. The speed factor was thus localized within the individual item without looking for distractions or appealing to gross introspection. In an attempt to discover if the differences in respect to speed inhered in a simple act, each observer was given one hundred and ten reactions of the muscular type by using the Sanford chronoscope with a visual stimulus. The authors state, "to our great surprise even inspection shows that these reaction times give individual differences similar to the time differences obtained in the items of the Alpha and of the Otis Tests." The correlations between reaction time and the time of the test items are with Alpha .70 and with Otis .90. The authors therefore conclude "that speed of reaction is an important and probably the most important factor in individual difference in the intelligent act." In other words they find that speed in performance is not only constant for different kinds of acts but it is very closely related to intelligence as tested by the tests. "We find that these differences in speed are not due to gross distractions or irrelevant acts, but inhere in a single item of

an intelligence test and probably in so simple an act as the muscular reaction."

Another study of importance is that of Sisk (30) in 1926 on the inter-relations of speed in simple and complex responses. Sisk was concerned with the questions of whether persons who are relatively slow or fast in simple reactions are also relatively slow or fast in complex reactions and what relation, if any, exists between the speed in different kinds of reaction tests and the score made on the Army Alpha test. Sisk secured the data of this investigation by giving eleven different tests of varying degrees of complexity to one hundred college students. Approximately three and one-half hours was the average time required for completing the eleven tests. Sisk concludes from the inter-correlations that there is no evidence for one who is quick in a simple reaction to be quick in a complex reaction, that there is only a slight tendency for one who is quick or slow in a complex reaction to be relatively quick or slow in another, and that the ability to make a high score on Alpha seems to be related to a small extent to ability to react to a complex situation.

E. C. Gilbert (16) has recently dealt with the constancy of speed in the performance of various tasks. The investigation is divided into two parts, the first being a laboratory study with four observers and the second an individual group study of sixteen girls between the ages of nine and thirteen. In each of the twelve tests the items were presented and timed separately and only those which were correct were included in the results. Accuracy was thus kept constant and speed, to this extent, isolated. In summarizing the results of this study Gilbert says that for the four observers in the laboratory study the tendency for positive correlations based on the ranks would seem to support the theory that speed is a constant factor. She points out that the reliability of the ranking obviously depends upon the differences between the means which she finds to be in the most part significant. Gilbert concludes her study by stating "that it seems evident that for the different tasks set speed relationships within a group are relatively stable. To the extent that these tests are representative of intellectual performance in general it may be concluded that the person who is quick in one task will be quick in others also."

The most recent contribution is the preliminary report by Travis (32) of his attempt to determine the relationship between the factors of intelligence and reflex time or speed in conduction of the nerve impulse in a reflex arc. While studying certain reflexes during stuttering among patients widely different in intelligence, Travis

found an apparent relationship between reflex time and mental ability. A technique was worked out so as to measure (in sigma) the length of time elapsing between the stimulation of the patellar tendon and the production of the action current on the muscle. Forty-four individuals ranging in mental ability from feeblemindedness to superior university students were used as subjects. The correlation between form A of the Otis test and reflex times was found to be .87. The results were so astounding that they were verified by another group of 43 college freshmen who presented a normal distribution of mental characteristics. In this case the correlation between the University of Iowa Qualifying Examination and reflex times was also .87. The correspondence between intelligence ratings and reflex times was very close for widely separated individuals as well as for intermediate subjects and in one individual the conduction of the impulse in the reflex arc was two and one-half times as fast as conduction over the same reflex arc in another individual.*

Probably the chief feature of this review of the rôle of speed in mental ability is the fact that the evidence is not clearly defined either for or against the existence of general speed ability. The broad assertions which lacked precise experimental foundations as well as the uncertainty and contradiction of results, ranging from positive denial of a relation to a positively high correlation between speed and ability, by equally competent investigators on both sides, suggest the need for further discriminating research. An attempt will be made to review a number of the more important indications and impressions gained from the literature by way of summary:

The earlier researches, where sensation was the chief classification for the study of reaction time, best illustrated by the study of Henmon (18), indicate that a measure of the reaction time is a measure of the ability in that sense category. These earlier contributions in the field of sensory reaction times are significant in that they pointed a way toward mental measurement via the reaction time method. After 1900 one finds a new tendency which departs from the sensory reaction time toward the emphasis on speed in relation to mental ability. Positive evidence for a relationship between school "brightness" and quickness of reaction was typified by the studies of Gilbert (15) and Bagley (3) and negative evidence was brought

* Still more recently there has come to the author's attention the monograph by Rounds (28a), indicating a relationship between speed in mental operations and the time of the Achilles tendon reflex. Unfortunately it is possible now only to add this brief note in proof.

forth by Wissler (**34**) and Whipple (**33**). These earlier studies with the possible exception of Whipple's were marked by laxity of procedure, subjective judgments, and lack of refinement in technique. These facts lead one to doubt the validity of the results whether they favor or deny the existence of general speed ability.

The series of investigations carried on from 1910 to 1920, which attempted to throw light on the speed of reaction more directly in relationship to ability as tested by the mental tests, are indeed very confusing. Burt (**8**), for example, found significant positive correlations, Brown's (**7**) results lack uniformity, Wyatt's (**35**) correlations averaged .40 and .45, McCall (**25**) obtained many significant positive correlations but upholds the use of "power" tests rather than "speed" tests in measuring intelligence. Anderson (**1**) wishes to rely more upon the quality than upon the speed of a mental response. There seems to be a high correlation between rates of performing different simple tasks, for Brown obtained correlations of .80 to .82 (**7**) and Gates .95 (**13**). These series of investigations were conducted primarily with school children as subjects, usually in groups without individual methods of timing either subjects or items. On account of these gross methods of procedure one is not surprised at the lack of uniformity of the results. In the series of researches where a special function was studied in the relationship of speed to comprehension or to quality, such as reading in the cases of Judd (**22**) and Gates (**13**) and handwriting in the case of Gates (**12**), the evidence indicates the positive significance of the rate factor.

The two extensive studies carried on with groups and the Army Alpha Test, namely, those of May (**24**) using 510 army recruits as subjects and Ruch and Koerth (**29**) using 122 University of Iowa students as subjects, decidedly do not uphold the claims of the slow but accurate person. Although some doubt may be thrown on these results due to the fact that Army Alpha is not a speed test for all subjects, the results clearly indicate that ability in respect to speed is an important factor in mental tests like the Army Alpha for subjects ranging from the average army recruit to university students.

The work of Hart and Spearman (**17**) and Bernstein (**4**) tends to deny the existence of a general speed factor in intelligence. It has been pointed out, however, that in both of these studies the authors were trying to find a "group" factor of speed independent of General Intelligence. The gross thing they call speed arrived at through an elaborate mathematical treatment is quite a different

thing from the speed factor of more recent investigators obtained through individual methods of timing test items. The more elaborate methods tend to confuse the issue and the analysis is not pushed back far enough into each individual response.

In the six more recent studies of the past four years where investigations have been conducted under carefully controlled conditions, the evidence, although contradictory, decidedly tends to favor the existence of a positive relationship between rate and ability in mental tests. Highsmith (20) and Sisk (30) conclude in general against the importance of speed although many of their correlations are positively significant. Clark (9) and Hunsicker (21) find positive evidence for the relationship of rate and mental ability, as do Peak and Boring (27) and Gilbert (16). Travis (32) finds a very high correlation between mental ability and rate of conduction of the nerve impulse in a reflex arc. These more recent researches have been conducted, for the most part, by the individual method of timing and have conformed to scientific testing procedure. By using these methods one finds greater constancy in the relation of mental ability to speed and the results should be more reliable indices of its significance. It should be clearly noted that the more refined and objective the investigation the more convinced the experimenter becomes in each case above of a vital relationship between rate and mental ability as tested by the intelligence tests. The assertions concerning the amount and nature of that relationship however have become less dogmatic. It is evident, therefore, that further research confined to laboratory technique is necessary in order to clear the issue and to establish the negative or positive significance of this important psychological problem.

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SOME CONTRIBUTIONS OF GESTALT PSYCHOLOGY FROM 1926 TO 1927

BY HSIAO HUNG HSIAO

Berkeley, California

It is an indisputable fact that, so far as English literature on the psychology of Gestalt is concerned, Helson's articles are the only ones that contain a rather comprehensive outline of the developments of the new psychology up to the year 1926. As to its latest contributions, no attempt has, as yet, been made to introduce them into the English-speaking world. Such an attempt, I believe, would be desirable in view of the fact that only a limited number are able to read the original language. In another article entitled "A Suggestive Review of Gestalt Psychology" (shortly to be published in the *Psychological Review*), some of these contributions have already been touched upon. Among the rest, the following will be of general interest to students of psychology.

The Effect of Experience upon the Perception of Figures. K. Gottschaldt (1) carried out a series of experiments to determine whether a figure "a" which was contained as a dependent part in a comprehensive figure "b" would be seen more easily if it was at first frequently presented as an independent whole (more than 500 times). His results are the following:

Plan I, which was preceded by three impressions, and in which no instruction to find "a" in "b" was given, produced very few positive cases. Only in 1.1 per cent of 92 tests, "a" sprang up spontaneously; and in 5.5 per cent, it was supplementarily found.

Plan II was the same as Plan I except in the fact that the number of impressions was increased to 520. Here just as low percentages were obtained. "A" sprang up only in 1.7 per cent of cases, and was supplementarily found in 1.2 per cent. This shows that the extremely reinforced impression of "a" led to no increase of positive cases. With 7 out of 11 subjects, no positive cases occurred.

Plans III and IV bore the same relationship to each other as Plans I and II with the additional instruction to actively search for "a". Through these instructions the positive cases were increased to about 24 per cent.

The impressions of 540 previous presentations remained without remarkable results. "A" sprang out with Plan III in 2.2 per cent of cases, and with Plan IV in 2 per cent; while it was supplementarily found with Plan III in 19.3 per cent and with Plan IV in 17.4 per cent.

The next step was to discover the relationship between the contribution of the different "b" figures to the positive cases and the strength of the Gestalt connection. In order to find out the different degrees of connection of "a" in the different "b" figures, special experiments with the instruction to look for "a" but without preliminary impressions were carried out with 112 subjects. As a result, the different "b" figures were arranged in the order of difficulty. Then this series was divided into five groups, namely, relatively very easy, easy, middle, difficult, and very difficult; and, in relation to these, the frequency of positive results in Plans I, II, III and IV was considered. It was found that the spontaneous springing up of "a" was in general limited to the two easy groups, that the positive cases increased, in general, with the decreasing strength of Gestalt connection, and that they failed completely with the strongest connections.

Studies of Color-Experiences. Th. Schjelderup-Ebbe (2) has experimentally established the fact that, with regard to its contrasting effects, the colorless quality is subject to a lability similar to that of the indifference-point of temperature sensation. Instead of treating black and white as a pair of antagonistic sensations, they ought to be looked upon as a series of saturations, if their relationship in the contrasting phenomenon of black and white is considered. From an entirely different starting point, the experiments of F. Donath (3) have conclusively shown that the colorless qualities do not belong to the sphere of color sensations, but obey their own laws.

Another fact that has been quantitatively established by Schjelderup-Ebbe (2) is that, for each color, the strength of contrast is approximately proportional to its saturation. In this connection, it is surprising to note that the long-waved colors, which are known as warm colors, with increasing saturation, have a distinctly less reinforcing effect upon the contrast than the cold colors. In this respect, the red is the weakest. Between it and the green colors stands the violet, to which a discordant feeling tone was attributed by Wundt. The greens that were used in this study were indifferent in feeling tone, but their contrasting effects were very strongly dependent upon the degree of their saturation.

Donath's work (3) is an endeavor to penetrate deeper into the

psychological nature of color saturation. The main result of this investigation is that the sensitivity to the difference of saturation does not obey Weber's Law. The saturation series of a color corresponds in no way to a simple decrease or increase of a quantity accompanied by an invariable quality; but, in view of the facts discovered, such a series ought to be regarded as composed of two series of quantitative changes which are directed against each other, (1) the series of brightnesses and (2) the series formed by the quantity of the color. As to other results, like those of Schjelderup-Ebbe, they seem to indicate that every simultaneous contrast of colors may be traced back to the contrast of saturations.

The studies of F. Ehrler (4) on color memory have shown that, with the mixture of two pure color qualities, the process of recognition was always turned to the brighter and especially to the emotionally exciting components—most regularly to the conspicuous yellow. The temporal interval between the original sense impression and the memory process determines, to a surprisingly little extent, the exactness of recognition. The vital factor is the total character of the experience complex, particularly, the direction and structure of the experience series in which the part-quality to be recognized is imbedded. These facts throw some new light upon the theory of feelings and other phases of the total psychic events in general.

The Influence of Need upon Retention. In studying the influence of need upon retention, B. Zeigarnik (5) gave his subjects a series of tasks to perform, which were interrupted at various periods. After the last task was given up, they were required to recall what they had done. The results of these experiments may be summarized as follows:

(1) The uncompleted tasks are better retained than the completed ones.

(2) The tasks that were interrupted at first but later completed are not better, but more poorly, retained than the uncompleted ones.

(3) The better retention of the uncompleted tasks is neither due to any emotional stress that arises through interruption nor due to the belief of the subjects that the experimenter wishes to have the interrupted tasks completed, but due to the mere fact of the completeness or incompleteness of the task.

(4) The tasks that are externally finished but internally uncompleted are well retained. Such tasks are those with whose solution subjects remain unsatisfied, those which one doubts one's ability to perform, and those that allow several possibilities of solution, and

those that are of special interest to individual persons. On the other hand, the tasks that are externally uncompleted, but internally completed are poorly retained. These are the tasks the partial completion of which is just as good as their total completion, those that are voluntarily given up by subjects and those that are hopelessly destroyed through interruption.

These facts are explained by the experimenter in the following way: At the very moment when the tasks are taken up, a quasi-need develops, which tends to be satisfied. It is the unsatisfied need that makes uncompleted tasks better retained. This assumption is supported, not only by quantitative results, but also by other facts, *i.e.*, the resistance of the subjects against interruption and their tendency to resume the work.

Dynamically speaking, this process corresponds to a tightened system which tends towards relief. The completion of a task represents the relief of the system, the satisfaction of a quasi-need, while the interruption of a task leaves a part of the quasi-need unsatisfied.

The Influence of Feeling upon Retention. Retention was studied by Würdemann (6) from another angle. In this experiment, smelling stuff was used to produce feelings; and tactile, olfactory, and color impressions were in different ways combined with simple stereometrical bodies to form complex experiences, which were to be recalled on the presentation of these various kinds of part-stimuli on later occasions.

The main results are: that experiences that are characterized by the great intensity or special depth of feeling remain longer in memory than those that are accompanied by weak feelings, or feelings of indifference, and that certain special feeling qualities likewise make retention better and longer.

To explain this favorable influence of feeling upon retention, the author says that feeling is a complex quality of the total content of consciousness, and that, at least in our waking states, there are no moments of consciousness that are perfectly free from feelings. Of course, the feeling state is subject to a continual rapid change as every change in the content of mind influences the quality of the total content of consciousness. It is the characteristic quality, great intensity or special depth of a feeling state or a volitional attitude that causes a considerable lingering of certain events in consciousness. Through this process of lingering, definite "part-complex qualities" arise; and those whose quality, intensity or depth is of the highest

degree dominate the total quality. The events to which these part-complex qualities are attached may be thereby recalled.

The Formation and Transformation of a Habit. The processes of habituation and rehabituation have been carefully studied by G. Schwarz (7). The procedure of his experiment may be briefly described as follows: A ball of 1 cm. diameter is to be thrown into three funnels, and runs over a groove into a box. There it remains lying until the handle of the box is pressed, and then it rolls over another short groove to be caught by the subject.

This performance consists of three chief parts. The initial performance is the selection of a ball out of a vessel; the middle performance includes throwing the ball into the funnel, pressing the handle, and catching the ball; and the end-performance consists in laying the ball on a model. The initial performance and the end-performance are collectively designated by the experimenter as "frame-performance" (Rahmen-handlung).

The plan of the experiment falls into the following four stages:

(1) The Stage of Habituation—the total performance is to be practiced until a habit is firmly established in the middle performance. However, the mechanization of the frame-performance is prevented by regularly increasing its difficulty.

(2) The Stage of the Transformation of the First Habit—The apparatus is so changed that the handle which has been pressed hitherto is now to be raised, and the frame-performance is at the same time made difficult.

(3) The Stage of the Restoration of the First Habit (Rückgewohnung)—Through another modification of the apparatus, the handle is now again to be pressed. The difficulty of the frame performance is again increased.

(4) The Stage of the Restoration of the Second Habit (Erneute Umgewohnung)—The handle is again to be raised, and at the same time new instructions are given regarding the frame-performance.

The results have shown that, if one practices for a time a frame-performance in which a middle performance is imbedded, and then changes a part of the middle performance, a wrong act will appear or tend to appear (*i.e.*, in the stage of habit transformation). Wrong acts or tendencies to such will also occur on returning to the original middle performance and during the further change of the middle performance.

These wrong acts, according to their phenomenological condition as well as according to their way of appearing, may be classified into

relapse-mistakes (Rückfall-fehler) and confusion-mistakes (Verwechslungs-fehler).

The relapse-mistakes arise only under the following conditions. First, the change must concern a dependent part of a "comprehensive performance." If the change takes place before the formation of the performance-whole (Handlungsganzheit), no relapse will appear. Second, the part to be changed must not be the principal part of a performance. Third, the changed part must have a single-coursed (ein-gleisige) structure, *i.e.*, it must not be one of two variants, which have occurred in previous experiences. Fourth, in experiments on habit that are not deeply rooted, the interval between the original performance and the changed one must not be too long.

During the restoration of the first habit, a tendency to relapse to the second habit appears; but it is far weaker, and, as a rule, does not lead to an overt act. This weakness is due to the fact that the subject has executed the performance in the preceding stage as one of two possible variants. The performance has thus acquired a two-coursed structure. Now, if the process autochthonously tends towards the second course, it will not be difficult to check it. For the same reason, during the restoration of the second habit, only a very weak tendency to relapse occurs.

In overcoming the tendency to relapse, the subject has to resort to one that is opposed to it. If the total performance is to occur as an automatic process, then wrong acts are sure to appear.

As to the danger of confusion, it differs from the tendency to relapse in that, previous to the performance, there is a sense of uncertainty which demands consideration. If consideration fails, then there is the possibility of a wrong act. This danger of confusion occurs only when the performance has acquired a two-coursed structure, *i.e.*, a structure having two variants. The critical point lies, not directly at the spot where the part-performance is affected by the danger of confusion, but at the place where this part-performance is introduced, namely, at the funnel. Therefore, in removing the danger of confusion, the subject has to control this introductory performance partly by changing the center of the total performance and partly by making the introductory performance relatively independent.

The Psychology of a Bird. M. Hertz (**8**) made quite a long study of the form-seeing capacity of the raven by the method of training, and reported his observations as follows:

It is the optical experiences that determine the behavior of the raven. At a very early stage of its development, the animal seems

to be able to see objects as such. At first, the boundaries of objects attract its attention and beak-activities. Everything destructible is destroyed and everything movable is moved. Not only individual objects are distinguished from one another, but groups of objects are also distinguished as such. They are grouped according to practical conceptions, *i.e.*, maize with maize, little stones with little stones, nut-shells with nut-shells, etc. As to the masked objects, the degree of similarity is graduated in the same way as with the human. The conception of similarity is independent of the presence of identical elements, but is based upon the character of the surface of things.

Although its own activities constantly bring it into new situations, the animal never looks helpless or nonplussed. Foolish behavior is seldom observed; and, when it occurs, there is always clear evidence that a definite complex has failed to be understood. Its behavior with those forms with which contrary experiences have been formerly connected is an instance in point.

Besides those acts that arise from the native organization of the animal and the general nature of the situation, there are acts which are determined by the situations of the moment. These should be regarded as acts of insight. The best performances of this kind seem to lie in the direction of searching and removing things.

In the range of situations for which the possibility of understanding may be assumed, the bird shows an excellent memory and a high capacity for learning. A specially good impression is made by the carrion crow in the constantly growing perfection in the way of preparing food. For each of these articles of food: meat, dogcake, biscuit, cedar nuts, sunflower seeds, etc., it has learned a special method of preparation, which is decidedly of great perfection. Those experiences with which the first solution cannot be independently made owing to lack of insight, such as the handling of doors, are acquired with great facility, and controlled with insight in the further course of behavior.

In this article, I have summarized the results of the experiments on color experiences, the psychology of a bird, the formation and transformation of a habit, the influence of need and feeling upon retention and the effect of experience upon the perception of figures. Since the purpose of this article is merely to furnish some information regarding the latest developments of the new psychology, I will do no more than call attention to the following few principal facts: (1) The study of color experiences seems to furnish a rich field for Gestalt psychology. (2) Hertz's observations of the raven have confirmed

some of Köhler's findings. (3) The study of habit as an organic unit throws some new light on the method of analysis. (4) The relation between retention and the need of the organism has been shown in its new phases. (5) The new conception of feeling not only harmonizes perfectly well with the fundamental notion of Gestalt, but also seems to be a workable one. (6) The importance of Gestalt in determining visual perception has been here clearly demonstrated.

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PSYCHOLOGICAL NECROLOGY (1903–1927)

EDWIN G. BORING
Harvard University

II

In the May number of the BULLETIN [1] I had the temerity to print a necrology of psychologists and near-psychologists for the last quarter of a century. I had no illusions as to its completeness or as to its perfection in accuracy, but I was entirely unaware at that time that the *Archives de psychologie* have since 1915 been printing necrological notices, varying from a line to even an entire page. There are 13 such lists in the *Archives,* and they include altogether 320 notices.[2] The first list covers briefly the century up to 1915. The other lists are more complete, although Professor Claparède, who was kind enough to call my attention to my lapse, has not always succeeded, any more than I, in being complete even as to all the fundamental data. Nevertheless the lists in the *Archives* give a great deal of information which should on occasion prove very valuable.

Scattered lists are, however, awkward to use unless cumulated every once in a while. Accordingly it seems worth while to publish now a supplementary list within the same volume of the BULLETIN. This list is based entirely upon the *Archives,* and in preparing it I have acted as little more than an arbitrary and, I hope, intelligent clerk.

From Claparède's total list of 320 I have eliminated 114 names of philosophers, physicians, zoologists, sociologists, and historians whom I had reason to believe from his comment or from my own knowledge made little or no contribution to psychology. I have,

[1] *Psychol. Bull.,* 1928, **25** (this vol.), 302–305.

[2] The necrologies for the years stated, occur as follow: For 1901–15: *Arch. de psychol.,* 1915, **15**, 396–398. For 1916: *ibid.,* 1916, **16**, 193–196. For 1917: *ibid.,* 1917, **16**, 375f. For 1918: *ibid.,* 1918, **17**, 150–152. For 1919: *ibid.,* 1919, **17**, 344. For 1920–21: *ibid.,* 1921, **18**, 189–192. For 1922: *ibid.,* 1922, **18**, 271f., 352. For 1921–23: *ibid.,* 1924, **19**, 95f. For 1923–25: *ibid.,* 1925, **19**, 272–276, 364. For 1925–26: *ibid.,* 1926, **20**, 262. For 1926–27: *ibid.,* 1928, **21**, 109–111.

however, retained physiologists, neurologists, and psychiatrists, thus using this opportunity to extend the scope of my first list, which was not ambitious for completeness in these fields. I have also eliminated eight names of men who died before 1903, the limiting date of my list. There remain, therefore, 198 names.

Among these 198 there are 66 that duplicate my first list. Thus there are 50 in my first list that Claparède did not have, and there remain after my eliminations 132 from Claparède's lists that I reprint below. On these data I have undertaken no research, but have accepted the word of the *Archives*.

In self-defense I should like to say that I did not miss the names of 132 psychologists. The physiological and psychiatric categories, swelled by the French influence, make up a large portion. Nevertheless I missed some names important for my own classification. In humility I should like to add further that I reprint many names of which I have never heard, in the belief that the knowledge of the users of these lists ought not to be limited by my own ignorance.

SUPPLEMENTARY LIST

Abraham, Karl, Berlin, December 25, 1925, age 49.
Abramowski, Edouard, Warsaw, June 22, 1918, age 46.
Alrutz, Sydney, Upsala, February 14, 1925, age 56.
Arnaud, François Leon, June 6, 1927, age 68.
Arréat, Lucien, Paris, 1922, age 81.
Baade, Walter, Göttingen, April 29, 1922, age 40.
Baginsky, A., Berlin, May 15, 1918, age 75.
Ballet, Gilbert, Paris, March 16, 1916, age 62.
Bechterew, Vladimir M., Leningrad, December 27, 1927, age 70.
Bianchi, Leonardo, Naples, February 13, 1927, age 79.
Boeuf, Marie, Paris, 1907, age 39.
Bois, Henri, Montpellier, September 11, 1924, age 62.
Bourneville, Paris, 1909, age 69.
Breuer, Joseph, Vienna, June 1925, age 83.
Brissaud, E., Paris, 1909.
Brodman, Korbinian, August 22, 1918, age 50.
Bunge, Gustav v., Basel, November 5, 1920, age 76.
Camus, Jean, Paris, December 1924.
Charpentier, Augustin, Nancy, September 1916, age 64.
Chaslin, Philippe, July 26, 1923, age 66.
Chatelain, Auguste, Neuchâtel, November 24, 1923, age 85.
Chauveau, J. B., Paris, January 1917, age 89.
Coué, Emile, Nancy, July 2, 1926, age 69.
Crocq, Jean, Belgium, January 3, 1925, age 56.
de Cyon, Elie, Leningrad, 1912, age 69.
Dastre, J. A., Paris, October 23, 1917, age 72.

DÉJERINE-KLUMPKE, AUGUSTA, Paris, November 5, 1927, age 68.
DUBOIS, PAUL, Bern, November 4, 1918, age 69.
DURKHEIM, EMILE, Paris, November 15, 1917, age 58.
EDINGER, LUDWIG, Frankfurt, January 16, 1918, age 62.
EMMINGHAUS, H., Freiburg, 1903, age 60.
ESPINAS, ALFRED, February 24, 1922, age 77.
FABRE, J. HENRI, Serignan, 1915, age 92.
FOIX, CHARLES, March 25, 1927, age 45.
FOUILLÉE, ALFRED, Menton, 1912, age 74.
GEHUCHTEN, A. VAN, Louvain, 1914, age 53.
GODFERNAUX, A., Paris, 1906, age 41.
GOLGI, CAMILLO, January 21, 1926, age 82.
GOWERS, WILLIAM, London, 1915, age 70.
GRADENIGO, GIUSEPPE, Turin, 1926, age 67.
GRASSET, JOSEPH, Montpellier, July 7, 1918, age 69.
GREEN, J. A., Sheffield, 1922.
HERR, LUCIEN, Paris, May 17, 1926, age 63.
HERZEN, ALEX, Lausanne, 1906, age 67.
HIS, WILHELM, Leipzig, 1904, age 73.
HITZIG, ED., Berlin, 1907, age 67.
HODGSON, RICHARD, Boston, 1905.
HÖFLER, ALOIS, Vienna, February 26, 1922, age 68.
HOFMANN, FRANZ B., June 6, 1926, age 57.
HORSLEY, VICTOR, July 16, 1916.
HOUTIN, ALBERT, Paris, August 1, 1926, age 59.
HUG-HELLMUTH, HERMINE, Vienna, 1924.
IMBERT, A., Montpellier, 1922.
INGENIEROS, JOSÉ, Buenos Aires, October 31, 1925, age 48.
JACKSON, HUGHLINGS, London, 1911, age 76.
JAVAL, EMILE, Paris, 1907, age 68.
JENDRASSIK, ERNST, Budapest, December 21, 1921, age 63.
JERUSALEM, WILHELM, Vienna, June 15, 1923, age 69.
JUQUELIER, April 14, 1920.
KARMIN, OTTO, Geneva, April 7, 1920, age 38.
KÖLLIKER, A. v., Würzburg, 1906, age 89.
KURELLA, HANS, Breslau, October 1916, age 53.
LADAME, PAUL, Geneva, October 21, 1919, age 77.
LAMPRECHT, KARL, Leipzig, 1915, age 59.
LANGE, CONRAD, Tübingen, August 1921, age 66.
LANGLOIS, J. P., Paris, June 1923.
LAPIE, PAUL, Paris, January 24, 1927, age 58.
LAY, WILHELM AUGUST, Karlsruhe, May 9, 1926, age 63.
LAZURSKI, A. F., Leningrad, 1917, age 53.
LECLÈRE, ALBERT, Bern, December 17, 1920, age 53.
LIEBEAULT, A., Nancy, 1904, age 81.
LIÉGLOIS, JULES, Nancy, 1908, age 75.
LIEPMANN, HUGO, May 6, 1925, age 61.
LOMBROSO, CÉSAR, Turin, October 19, 1909, age 73.
LÖWENFELD, LÉOPOLD, Munich, December 20, 1923, age 76.

LUBBOCK, JOHN, London, 1913, age 79.
MAGNAN, VALENTIN, Paris, September 1916, age 81.
MAHAIM, ALBERT, Lausanne, March 29, 1925, age 58.
MANTEGAZZA, PAOLO, Florence, 1910, age 79.
MAREY, E. J., Paris, 1904, age 74.
MARRO, ANT., Turin, 1913, age 73.
MAUDSLEY, HENRY, January 24, 1918, age 82.
MERCIER, DÉSIRÉ, Malines, January 23, 1926, age 74.
MIGNARD, MAURICE, August 2, 1926, age 45.
MILLIOUD, MAURICE, January 8, 1925, age 59.
MITCHELL, WEIR, Philadelphia, 1914, age 83.
MÖBIUS, P. J., Leipzig, 1907, age 54.
MOREAU, PAUL, Paris, 1908, age 64.
MOTEL, Paris, 1909, age 77.
MOURLY-VOLD, J., Oslo, 1907, age 57.
MÜLLER-LYER, FRANZ, Munich, October 29, 1916, age 59.
MUNK, HERMANN, Berlin, 1912, age 72.
MURISIER, ERNEST, Neuchâtel, 1903, age 36.
NATORP, PAUL, Marburg, August 17, 1924, age 79.
OCHOROWICZ, JULIEN, May 1917, age 67.
OPPENHEIM, HERMANN, May 22, 1919, age 61.
PENTA, PASQ., Naples, 1904, age 45.
PÉREZ, BERNARD, Paris, 1903, age 67.
PFLÜGER, E. F. W., Bonn, 1910, age 80.
PILLON, FRANÇOIS, Paris, 1914, age 84.
PRANDTL, ANTONIN, Würzburg, December 25, 1927, age 47.
PRÉVOST, JEAN LOUIS, Geneva, September 12, 1927, age 89.
RANKE, JOHANNES, Munich, 1916, age 80.
RAUH, FRÉD., 1909, age 48.
RAYLEIGH, LORD, London, June 30, 1919, age 77.
REGIS, EMMANUEL, Bordeaux, June 1918, age 63.
ROFFENSTEIN, GASTON, Vienna, September 7, 1927.
RORSCHACH, HERMANN, Herisau, April 2, 1922, age 37.
ROSSI, PASQ., Cosenza, 1905.
SAFFIOTTI, UMBERTO, Milan, October 20, 1927.
SCHNYDER, LOUIS, Bern, March 20, 1927.
SIEBECK, HERMANN, Giessen, February 22, 1920, age 78.
SIGHELE, SCIPIO, Florence, 1913, age 45.
SILBERER, HERBERT, January 12, 1923, age 40.
SIMARRO, LOUIS, Madrid, June 1921.
SOURY, JULES, Paris, 1915, age 73.
STRÜMPELL, ADOLF v., 1925, age 72.
TAMBURINI, AUGUSTO, Rome, July 28, 1919, age 71.
TARDE, GABRIEL, Paris, 1904, age 61.
TAYLOR, FRED. W., New York, 1915, age 59.
TIGERSTEDT, ROBERT, December 1923, age 70.
TREVES, Z., Milan, 1911, age 42.
VALLON, CHARLES, 1924, age 71.
VAN LISZT, FRANZ, June 21, 1919, age 68.

VARENDONCK, JULIEN, Gand, June 11, 1924.
VERWORN, MAX, Berlin, November 23, 1921, age 58.
VIGOUROUX, AUG., December 1, 1918.
VIEQUEIRA, J. V., Coruna, September 1924.
VITTOZ, ROGER, Lausanne, April 11, 1925.
WALLER, AUG., DÉSIRÉ, London, March 11, 1922, age 66.
WEISMANN, AUG., Freiburg, 1915, age 81.
ZIEGLER, HEINRICH ERNST, Stuttgart, June 2, 1925, age 66.

In the 66 cases where Claparède and I have duplicated each other, there are a number of instances where we diverge by a day in the date or by a year in the age, and where I can not resolve the conflict, since my source says one thing and Claparède says another. There were only eight cases of wider divergence, and in none of these have I been able to substantiate the statement of the *Archives*, except in the case of Hyslop's age. One age is definitely a controversial matter.

There were also 15 cases where the data of the *Archives* were more complete by some one datum than mine. I therefore reprint these items below, with the additional datum added.

ADDITIONS TO FIRST LIST

BENUSSI, VITTORIO, Padua, November 24, 1927, age 49.
DÉJERINE, JOSEPH JULES, Paris, February 26, 1917, age 67.
FÉRÉ, CHARLES, Bicêtre, Paris, 1907, age 55.
HENRY, CHARLES, Paris, November 3 or 9, 1926, age 67.
HILLEBRAND, FRANZ, Innsbruck, April 13, 1926, age 63.
HYSLOP, JAMES HENRY, New York, June 17, 1920, age 66.
LEHMANN, ALFRED, Copenhagen, September 26, 1921, age 63.
MARTIUS, GÖTZ, Kiel, May 27, 1927, age 74.
NACCARATI, SANTE, Columbia Univ., August 12, 1926, age 39.
PICK, ARNOLD, Prague, April 4, 1924, age 75.
SULLY, JAMES, London, November 2, 1923, age 81.
VASCHIDE, N., Paris, 1907, age 33.
WAYENBURG, GERARDUS VAN, Amsterdam, May 18, 1926, age 64.
WERNICKE, CARL, Halle, June 15, 1905, age 57.
WITASEK, STEPHAN, Graz, April 13, 1915.

SPECIAL REVIEW

WELD, H. P., *Psychology as Science.* New York: Holt, 1928. Pp. xi+297.

This book differs widely from the usual run of textbooks and treatises. It is not a summary of scientific discoveries, but an exposition of the fundamental problems of psychology, and a critique of the conflicting viewpoints which prevail to-day.

Professor Weld starts out by comparing the popular or commonsense conception of nature with the traditional attitude of science toward natural phenomena and with the attitude of what he calls "critical" science. According to the author, science till recently has generally followed the popular conception; it has aimed to *explain* nature, not merely to *describe* its phenomena. In psychology this attitude characterizes most of our contemporary scholars and investigators. Despite the essential differences between (*e.g.*) McDougall, Watson, Woodworth, and Bentley, their attitude toward psychology "as science" is similar; their standpoint is *empirical* rather than *critical.* Empirical science seeks to discover not only the *what* but the *why;* and its aim is not merely discovery but prediction and control of nature. While the majority of contemporary psychologists treat the science empirically, Titchener and his followers adopt a rigidly critical method. They are not concerned with explaining phenomena, but only with observing and describing conscious experiences as these actually occur. Hence, the subject-matter treated by this school is called the *existential* psychology.

This discussion, which occupies the first four chapters, is of peculiar interest, since it seems to represent Titchener's own conception of psychology in a clearer way than has been brought out by Titchener himself. The reviewer has at times been puzzled by Titchener's unusual limitation of the term empirical psychology, and his evident repudiation of the empirical attitude. Weld's discussion clears up these difficulties.

Naturally there are likely to be vigorous protests from some of the ill-assorted bedfellows, and Weld's characterization of the "empirical" viewpoint may be challenged. Between the "what" and the "why" lies the realm of the "how" which the author apparently ignores, and which Titchener himself seems to have treated inadequately. Science is concerned with sequences, with the way in which events succeed one another, not merely with momentary phe-

nomena. Is not this also description? The existential psychology has stood out firmly against the causal explanation of sequences; this attitude is characteristic of Titchener's own work. Possibly it is for this very reason that the Cornell type of psychology has proved unsatisfying to psychologists at large. Those who find such principles as "conservation of energy" and "natural selection" illuminating, are not content with detached, snapshot views of conscious phenomena.

It is also too sweeping to attribute to all the writers cited, the view that the chief aim of science is to predict and control the course of natural phenomena. There are many (including the present reviewer) who deny this squarely. They regard prediction as a useful means of checking up scientific induction; they would agree with Weld that in any other rôle prediction and control belong to the realm of technology rather than science.

The distinction between description and explanation leads naturally to the contrasting of phenomena with meanings and values—the distinction between the world of fact and the world of appreciation, to use Josiah Royce's language.

"Titchener has shown," says Weld, ". . . that meaning is not intrinsic to the content-process, that meanings may be put on and off a process, that an experience and its meaning may be disjoined in time, that one and the same experience may have several meanings, that one and the same meaning may attach to several experiences, and that meaning and mental process are not covariants" (p. 62). The virtual elimination of values and meanings from the category of psychological data by the Titchenerians (p. 61) is likely to stimulate controversy. If meanings may attach to experiences, are they not actually phenomenological data?

The remainder of the book is a masterly discussion of the chief problems in the various branches of psychology. The author takes up in turn differential, abnormal, and animal psychology, child psychology, social psychology, and psychotechnology. He sketches the leading lines of investigation in each field and reviews the problems attacked up to the present. In these chapters the "empirical" method is treated rather more sympathetically.

The book gives one a clear insight into the achievements and contemporary status of psychology. It shows a keen appreciation of relative values and should appeal to the mature psychologist quite as fully as to the beginner for whom it is primarily designed.

Princeton University HOWARD C. WARREN

BOOKS RECEIVED

ROBERT S. ELLIS, *The Psychology of Individual Differences.* N. Y.: Appleton. Pp. xxiv+533.

ETIENNE RABAUD, *How Animals Find Their Way About.* (Trans. by I. H. Myers.) N. Y.: Harcourt, Brace, 1928. Pp. ix+142.

JOHN F. MARKEY, *The Symbolic Process and Its Integration in Children.* N. Y.: Harcourt, Brace, 1928. Pp. xii+192.

CLARK L. HULL, *Aptitude Testing.* Yonkers: World Book, 1928. Pp. xiv+535.

SIGMUND FREUD, *The Future of an Illusion.* London: Hogarth Press, 1928. (Internat. Psycho-Analytical Library, No. 15.) Pp. 98.

ARTHUR I. GATES, *Elementary Psychology.* (Revised Edition.) N. Y.: Macmillan, 1928. Pp. xvii+612.

WALTER POPPELREUTHER, *Psychologische Begutachtung der Erwerbsbeschränkten.* Handbuch der biologischen Arbeitsmethoden. Abt. VI, Teil C1, Heft 6. Pp. 369–552.

JEAN PIAGET, *Judgment and Reasoning in the Child.* N. Y.: Harcourt, Brace, 1928. Pp. viii+260.

WILLIAM M. WHEELER, *The Social Instincts.* Their Origin and Evolution. N. Y.: Harcourt, Brace, 1928. Pp. xviii+378.

E. HANBURY HANKIN, *The Cave Man's Legacy.* N. Y.: Dutton, 1928. Pp. 180.

JOHN B. WATSON, *The Ways of Behaviorism.* N. Y.: Harper, 1928. Pp. 144.

JOHN RICKMAN, *Index Psychoanalyticus.* 1893–1926. London: Hogarth, 1928. Pp. 276.

JAMES C. MANRY, *World Citizenship.* Univ. of Iowa Studies in Character. Vol. 1, No. 1, 1928. Pp. 67.

Sources of Information Concerning the Operation of the 18th Amendment. N. Y.: Social Science Research Council, 1928. Pp. 70.

PSYCHE CATTELL, *Dentition as a Measure of Maturity.* Harvard Mono. in Educ., No. 9, 1928. Pp. viii+91.